tium-like mycelium which surrounds the perithecium in the interior of the plant on which it lives reminds one of *S. Graminis*, Pers., of which the preceding may perhaps be the pycnidic form.

Previous observers of this fungus also have always found only this one form of spore, and regarded it, like myself, as a stylospore, and not as an ascus, which likewise might be justified*.

Desmazières first described such spores, in a Fungus which occurred upon *Alopecurus*, *Agrostis*, and *Holcus*, under the name of *Dilophospora Graminis* (Ann. Sci. Nat. sér. 2. tome xiv. p. 5, pl. 1. fig. 2 *a*, *b*, *c*). Fuckel found a species of the same genus upon *Holcus lanatus*, and published it as *Dilophospora Holci* (Bot. Zeit. 1861, p. 250). Berkeley afterwards detected a very similar fungus as the cause of disease in a wheat-field (Horticultural Journal, 1862, No. 5). The spores, however, are described by this mycologist as furnished with only three, perfectly simple, much broader, linear, pointed, and not filiform appendages at each end; whilst Desmazières describes and figures the spores observed by him as furnished with three, or even only two, filiform appendages, which were sometimes simple, but usually forked once or twice. Fuckel describes his *Dilophospora Holci* as breaking out from yellow spots on the grass-leaf—a peculiarity not presented by the *Holcus*-leaves examined by me.

Further observations must decide whether these differences express the peculiarities of different species of plants, or only *variations of one species*.

LII.—*Note on the Reproduction of the* Aphides.

By Professor E. Claparède†.

The reproduction of the *Aphides*, after having attracted the attention of so many distinguished men, has recently given rise to fresh investigations on the part of two observers, M. Mecznikow and M. Balbiani. The results at which these two naturalists have arrived show plainly that the subject was far from being exhausted. Each of them has worked independently. The first of M. Mecznikow's publications‡ is anterior by some months to the first communication of M. Balbiani to the Academy of Sciences in Paris (4th, 11th, and 25th June, 1866). Nevertheless the latter author seems to have had no knowledge

* Schlechtendal, Bot. Zeit. 1863.

† Translated by W. S. Dallas, F.L.S. &c., from the 'Annales des Sciences Naturelles,' 5e série, tome vii. pp. 21–29.

‡ "Untersuchungen über die Embryologie der Hemipteren," Zeitschr für wiss. Zool. xvi. p. 128.

of it, since he does not mention it in a subsequent, more detailed paper*. The differences between these two observers have become still more striking since the publication of a very elaborate memoir by M. Mecznikow†, accompanied by more than fifty figures relating to the embryogeny of the *Aphides*, and which is only the development of the note already cited.

On examining the publications to which I have just referred, it is easy to see that both M. Mecznikow and M. Balbiani have very conscientiously studied the objects that they had before them, and that in most cases they have seen exactly the same things. And yet what a distance there is between the final results at which they have arrived! A single word will suffice to make this intelligible: with M. Mecznikow the *Aphides* are agamogenetic; with M. Balbiani they are hermaphrodites.

How are we to choose between these opposite results, announced by observers apparently equally conscientious? The only way is evidently to take up the subject again *ab ovo*, and to submit all the divergencies to the touchstone of new and impartial observations.

This is what I determined to do by an investigation of *Aphis Rosæ*, of which the embryos are comparatively favourable for researches of this nature. The theory of the hermaphroditism of the *Aphides* is untenable. Its author, founding his opinion upon certain facts carefully observed, has evidently allowed himself to be carried far beyond the conclusions to which they could legitimately give rise. His meeting by chance with certain morbid phenomena has also perhaps assisted to keep him in the track into which he had strayed. I do not hesitate to assert that any one who will have the patience to resume carefully this minute investigation will be compelled, while rendering justice to the labours of M. Balbiani, to reject completely the consequences which that author has drawn from them.

The problem of the reproduction of the *Aphides* is very simply solved, according to M. Balbiani, in the following manner:— From the first moments of embryonic life the blastoderm gives origin to two juxtaposed cellular masses, one colourless, the other permeated by granulations which give it a green or greenish-yellow tinge. Of these two masses the first becomes an ovary, and the second a testis, in which are developed zoospermia in the form of *Amœbæ*. These zoospermia fecundate the ovary, the testis itself disappears, and the fecundated ovules

* Journal de l'Anat. et de la Physiol. 3e Année, No. 5, September and October 1866.

† "Embryologische Studien an Insekten. Die Entwickelung der viviparen Aphiden," Zeitschr. für wiss. Zool. xvi. p. 437.

commence their evolution even in the interior of the embryo still contained in the body of its mother; consequently there is neither alternate generation nor parthenogenesis.

The two cellular masses to which M. Balbiani ascribes so important a part in the reproduction of the *Aphides* really exist, as we may easily ascertain. M. Mecznikow has studied them with extreme care. The colourless one he regards as a blastogene or *pseudovarium*, so that he attributes to it the same physiological part as M. Balbiani. But the other, the green mass, the testicle according to M. Balbiani, is regarded in a very different light by M. Mecznikow; he gives it the name of *secondary vitellus*, because he considers it a magazine of material fitted to be assimilated in the course of the organogenetic process. We shall see that this latter interpretation is by far the most probable; but for this purpose it is necessary to go back to the origin of the matter.

The extremity of each compartment of the *pseudovarium* is occupied by numerous nuclei disseminated in a protoplasm. These nuclei are the germinal vesicles of the future ovules; in fact the lowest one becomes isolated from the others, and surrounds itself with a mass of protoplasm, in which refringent granules soon make their appearance: this is the ovule. M. Balbiani, adopting throughout his memoir M. Robin's theory of the production of cells by gemmation upon the periphery of a blastoderm, represents the ovules as originating by gemmation upon the surface of a central cell. M. Mecznikow nowhere mentions or figures this central cell of the pseudovarium, nor have I succeeded in discovering it. But, however this may be, as soon as a *pseudovum* attains maturity in the lower part of the compartment of the pseudovarium its evolution commences. We soon see, in the midst of the vitelline granules, several clear nuclei, very similar to what the germinal vesicle was originally. M. Mecznikow regards these nuclei as having been produced by the division of this germinal vesicle. Is he quite right upon this point? I cannot venture to decide*. What is certain is, that these nuclei multiply and advance to the periphery, where they are found lodged in a layer of protoplasm, constituting thenceforward a true blastoderm. This membrane, in fact, becomes cellular by a differentiation of the protoplasm, which groups itself in little masses round each of the nuclei.

M. Balbiani, indeed, represents matters in a very different light; but it is impossible for me to agree with him. He first

* In certain *Acari* I have convinced myself that the nuclei of the blastoderm result from the division of the germinal vesicle; but I reserve these observations for future publication.

of all makes the germinal vesicle disappear in a homogeneous vitellus. As to the mere fact of the persistance of the germinal vesicle, it is, I admit, very difficult to arrive at perfect conviction, because it is possible that the first nucleus, from which all the nuclei of the blastodermic cells are produced by division, might be itself produced spontaneously in the midst of the vitellus some time after the disappearance of the germinal vesicle. Therefore I do not venture to pronounce too absolute an opinion upon this point; but in any case it is not true that the vitellus is homogeneous at this period. On the contrary, it contains numerous granules collaterally with the germinal vesicle, as M. Mecznikow has very well shown; and as regards the formation of the blastodermic cells by gemmation at the surface of the vitellus (Robin's theory), as represented by M. Balbiani, I do not know how we are to reconcile it with the incontestable multiplication of the nuclei in the interior of the vitelline mass—a phenomenon to which I have just alluded.

The blastoderm formed surrounds the ovum, now become pyriform, over its whole surface, except at the inferior pole, as M. Mecznikow and M. Balbiani describe. The portion of the blastoderm in the neighbourhood of the inferior pole developes into a sort of cylindrical process, which is soon detached by a complete constriction, and separates from the embryo properly so called. This body, seen by both M. Mecznikow and M. Balbiani, has been very differently regarded by them. We shall not take any notice of it, as it plays no active part in the organogenic evolution.

From this moment the embryo presents an oval form, and is composed only of an external blastodermic layer and of a central vitelline mass. M. Balbiani calls this mass a *cell*. I regret to have to introduce here a discussion of words, but I cannot subscribe to this denomination. No doubt the investigations of MM. Brücke, Beale, Max Schultze, Häckel, and others have compelled us to accept a singular transformation of the nature of the word *cell*; but there is a long way from this to the confusion introduced into scientific language by M. Balbiani, a confusion to which I shall again have occasion to refer. With him, it would appear, the word *cell* is to be applied in histology to whatever has form; whilst with all histologists who still employ this term, the name *cell* can only be applied to a protoplasmic mass, which, at least during part of its existence, is furnished with a nucleus, with its well-known physical and chemical characters. Now the vitelline mass in question certainly has an ovoid form, since it is bounded by the blastoderm; but it possesses no nucleus, and consequently can on no account merit the name of *cell*. But we may pass over this technical point,

and the rather because, I repeat, the description of the blastoderm, as given by M. Balbiani, is correct in its chief points.

In consequence of a multiplication of the cells at the inferior pole of the blastoderm, this gives origin to a protuberance, which projects into the central vitelline mass. This protuberance gradually increases in size, and subsequently plays an important part in the organogenesis; but we may remark at once that, in proportion as the protuberance is developed, the vitelline mass diminishes by absorption, and finally even disappears completely.

One cell of the protuberance in question soon distinguishes itself from the rest by its green colour, due to the appearance in its protoplasm of a multitude of little coloured granules. This cell multiplies itself rapidly, giving origin in consequence to a mass of green cells, to which I shall apply the name of the *green mass*, so as not to prejudge its physiological value. It will be seen already that this is the *testis* of M. Balbiani*, the *secondary vitellus* of M. Mecznikow. At this same period of embryonic life a group of cells is seen to detach itself from the blastodermic protuberance and attach itself to the side of the green mass; and this will afterwards constitute the blastogene or *pseudovarium*, as both M. Mecznikow and M. Balbiani have proved.

I pass rapidly over these remarkable phases of organogenesis, because, with the exception of a few details, they have been represented in a very similar manner by the two physiologists who have led me to take up the pen; but at this point it is desirable to dwell upon some histological details, as upon these M. Balbiani has raised his theory, seductive but, I think, radically false, of the hemaphroditism of the *Aphides*.

According to M. Balbiani the cells of the organ in question, when once penetrated by the fine granulations which give them their green colour, generate in their interior a multitude of small, pale daughter cells, furnished with a membrane and with a nucleus, which he regards as cells of development of the spermatic elements. They are in fact soon replaced by innumerable small dark corpuscles of 0·001–0·002 millimetre in diameter, which, under a high power, appear "like very small *Amœbæ*;" but, adds the author, "their form does not seem to change under the microscope." "The mother cells," continues M. Balbiani, "have then lost their transparency and their green colour; they have become opaque and brownish, and readily

* M. Balbiani, properly speaking, represents this green mass as originating, not from the blastodermic protuberance, but from the cylindrical process, which, I have said, plays no active part in the development of the ovum. I do not see that I can agree with him on this point.

break up, resolving themselves into a sort of dust after the destruction of their enveloping membrane. In many *Aphides* these amœboid corpuscles undergo a further degree of evolution by their conversion into little unequal bacilli, which are straight or variously bent, immobile and colourless, and from 0·005 to 0·020 millimetre in length. One would easily be led to take them for a parasitic vegetable production, but for having witnessed all the successive phases of the transformation of these elements." (Balbiani, *loc. cit.* pp. 548, 549.)

These observations, and the interpretation which accompanies them, are of prime importance. They form the cornerstone of M. Balbiani's theory. If we look through the memoir of M. Mecznikow, otherwise so conscientious and full of details, we do not find a single word upon these phenomena. It would be an essential phase of development which has entirely escaped him.

Let us now see what can be learnt on this point from the Aphis of the Rose. The cells of the green mass, the boundaries of which are always very distinct, present a clear circular nucleus, 0·01 millim. in diameter, and furnished with a nucleolus. They generate in their interior a number of homogeneous spherical globules, among which may be distinguished a multitude of exceedingly fine granules. These spherical globules are the *daughter cells* of M. Balbiani. Indeed it would appear that, in his eyes, every granule is worthy of this name. Examined in every way, with the best objectives of Smith and Beck, and with the aid of Hartnack's immersion lenses, these globules did not show me anything which presented even a distant resemblance to a nucleus in the histological sense of that word.

Even supposing that, entering into the views of M. Balbiani, we assign the name and value of daughter cells to the globules in question, we shall still be far from the theory of hermaphroditism; for the metamorphoses which this physiologist represents them as undergoing cannot be regarded as normal phenomena. The green mass, in fact, by no means disappears, but persists *with all its characters long after the development of a new generation of embryos has commenced in the interior of the embryo*; moreover, as M. Mecznikow has also shown, it persists *during the whole life* side by side with the fatty body.

This first point, namely the persistence of the green mass, being established in opposition to the description of M. Balbiani, I find myself compelled to dispute the correctness of all that relates to the formation of spermatic elements. M. Balbiani's statement, moreover, is obscure and in contradiction with itself. Thus this observer tells us that the daughter cells are soon replaced by innumerable corpuscles, which appear like little *Amœbæ*; "*but their form*," he adds, "*does not seem to change*

under the microscope." Now is there anything characteristic in the *Amœbæ* except their mobility? The mode of movement alone distinguishes an amœboid body from a drop of albuminous substance. Has not M. Balbiani, preoccupied by the notion of finding zoospermia in the *Aphides*, recollected that in some animals (certain Nematode worms, for example) the spermatic elements have a form which has been designated as amœboid? If this be the case, he has forgotten that the mode of movement alone led to the application of such an epithet to these zoospermia.

Moreover I repeat that the asserted disappearance of the green mass, upon which M. Balbiani lays so much stress in order to give probability to its testicular function, does not occur. The green cells persist, each retaining its nucleus and preserving in its interior the spherical globules, the latter not being transformed into either amœboid or bacilliform elements. This we may ascertain simultaneously from different generations contained one within the other. This essential point may easily be verified by any one; and whoever will take the trouble will find all doubt dispelled from his mind on this point.

But how are we to explain M. Balbiani's statement? for in this case we have to do not only with a question of interpretation, but also with a question of fact. I think that M. Balbiani himself furnishes us with the means when he says that at the first glance he thought he had to do with parasitic vegetable organisms. This first impression was no doubt an *inspiration* in the theological sense of the word. A morbid state of the individuals investigated by M. Balbiani can alone account for the essential differences which distinguish this part of his observations from normal phenomena. In connexion with this it is not uninteresting to find that at Naples the Aphis of the Rose, and especially its *pseudovaria*, are infested by parasitic Mucedineæ.

The function of a secondary vitellus ascribed by M. Mecznikow to the green mass is at all events more probable than that of a testis. This organ may very well serve as a magazine of assimilable substance when the primary vitellus is absorbed. The analogies of the green mass to a vitellus, both in appearance and position, are at any rate so great that Mr. Huxley regarded it as a true vitellus. An objection to this view may be derived from the fact that the organ in question exists not only during the embryonic period, but also throughout life. However, it must be remarked that its relative importance diminishes gradually with age, and that consequently the objection loses much of its weight.

To sum up, the theory of the hermaphroditism of the

Aphides does not seem to me to rest upon any solid basis; and the general opinion which regards the most habitual mode of reproduction of the *Aphides* as a case of agamogenesis is alone true. I have, however, no pretension to claim, by the publication of this note, any scientific rights in connexion with the embryogeny of the *Aphides*. Those who have resumed the investigation of these singular phenomena at the point where Mr. Huxley had left it, and who have caused it to advance remarkably, are at present MM. Mecznikow and Balbiani alone. If I have taken up the pen, it is because there existed between these two observers such considerable differences upon one point, and that a fundamental one, that it was necessary to test their observations. But I feel perfectly that if, by these few lines, I assist in banishing an error from science, I nevertheless introduce no new fact. Definitively I leave matters where M. Mecznikow has placed them.

LIII.—*Remarks on M. Claparède's Note on the Reproduction of the* Aphides. By M. Balbiani*.

Although M. Milne-Edwards has had the kindness to communicate to me M. Claparède's note before its insertion in the 'Annales,' I do not think it necessary to reply at the moment to the objections which the author endeavours to raise against my interpretation of the mode of reproduction of the viviparous *Aphides*, or to some perfectly gratuitous allegations which his paper contains. I think this reply will be better placed in the memoir, accompanied by plates, which I propose shortly to publish upon the generation of the *Aphides*. There is only one point in M. Claparède's note which I think it essential to notice here, namely that relating to the priority which he seems to claim in favour of M. Mecznikow for all the facts upon which our observations present a more or less complete agreement.

It is certain that M. Mecznikow, three months before my communications to the Academy of Sciences, published some researches upon the embryogeny of the Hemiptera, which appeared, as a preliminary notice, in Siebold and Kölliker's 'Zeitschrift.' But in this paper, which occupies in all four pages of the journal in question, the author devotes only a little more than one page to the development of the *Aphides*; and here he omits most of the more characteristic facts in the embryogeny of those

* Annales des Sciences Naturelles, 5e série, tome vii. pp. 30–31.